ROYAL ASTRONOMICAL SOCIETY.

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CAPT. W. H. SMYTH, R.N., Vice-President, in the Chair.

Thos. C. Janson, Esq., F.L.S., Stamford Hill, Middlesex, was balloted for, and duly elected a Fellow of the Society.

FLORA.

Observations.

MAKERSTOUN. Equatoreal. (Sir T. M. Brisbane & Mr. Brown.)

Date. 1847.	Makerstoun M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Star of Comparison.	No. of Obs.	Hind's Ephemeris, — Obs. R.A. s	N.P.D. ″
Dec. 28	7 52 55	4 12 50·67	74 29 53·9	B.A.C. 1328	5	−0·64	−4·2
30	8 24 34	11 53·80	74 20 24·4	— 1366	9	0·66	6·3
	8 40 25	53·21	19·5	— 1380	6	0·35	4·5
1848.							
Jan. 5	10 23 47	10 0·35	73 49 37·7	— 1329	15	0·46	−1·7
7	8 40 13	9 42·55	39 16·9	— 1350	2	−0·29	+0·8
13	7 51 46	9 43·75		— 1356	10	+0·02	
	7 52 50		73 5 58·1	— 1356	6		−4·1
15	7 17 6	10 2·48	72 54 31·8	— 1346	4	−0·05	7·1
	7 41 28	2·67		— 1356	14	0·03	
	7 41 35		22·6	— 1356	8		3·9
18	7 20 44	10 47·83	36 43·4	— 1346	8	0·11	5·3
	8 6 31	48·41	31·9	— 1365	7	0·09	5·2
19	7 28 47		30 40·9	— 1365	5		4·7
	7 35 44	11 7·51		— 1365	9	0·09	
20	7 29 44	11 29·16		— 1365	10	0·12	
	7 45 5		24 34·6	— 1365	4		3·9
21	7 31 33	11 53·03	72 18 33·0	— 1365	5	−0·14	2·5
25	6 51 15	13 47·75	71 54 16·3	Rümker 1162	15	+0·25	4·5
26	7 22 39	14 22·52	47 57·7	— 1162	12	0·33	4·0
31	7 9 29	17 41·65	16 56·7	B.A.C. 1361	8	0·14	0·0
	7 34 58	42·28	52·1	— 1376	11	0·30	2·0
Feb. 2	7 52 21	4 19 15·70	71 4 20·9	— 1376	15	+(0·21)	−(3·2)

The observations are corrected for refraction and parallax.

The observations were made by Mr. John Welsh, with a spider's-line micrometer. The apparent place of the compared star has been computed from the catalogue referred to.

DURHAM. (Prof. Chevallier & Mr. R. A. Thompson.)

1847.	G.M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	No. of Comp.	Star.	
Oct. 27	10 37 35	5 3 52·73		5	*a*	
28	10 39 26	3 42·37		5	*a*	
Nov. 1	10 1 44	2 33·54	76 9 19·4	2	B.Z. iv. 1312	
2	11 40 59	5 2 8·63	10 7·9	5	— —	
9	9 58 47	4 58 19·47	13 21·5	8	— —	N.P.D. doubtful.
16	11 16 15	52 41·13	13 15·3	4	B.A.C. 1500	
19	13 2 23	49 46·28	11 53·9		Meridian.	
	12 11 0	48·56	53·4	7	B.A.C. 1500	
Dec. 3	12 5 2	34 41·25		6	— —	
	12 34 3		75 53 31·3	2	— —	doubtful.
11	10 33 48	26 8·13	75 34 13·4	8	— 1409	
1848.						
Jan. 5	10 23 49	10 0·41	73 49 42·1	7	— 1329	
11	8 21 5	9 34·29	17 16·5	2	— 1350	
13	10 37 48	9 44·63		7	— 1356	
	10 37 48	44·62		7	— 1346	
	10 21 18		73 5 25·2	7	— 1356	
26	8 32 14	14 23·93		9	H.C. 8125	
	8 26 20		71 47 39·2	8	— —	
28	7 48 57	15 37·11	35 25·3	7	B.A.C. 1361	
	10 57 10	42·04	34 38·8	1	— —	
31	6 51 52	17 40·59	17 5·4	8	— —	
	9 26 14	45·53	16 26·8	1	— —	
	10 27 49	47·23	16 11·4	4	— —	
Feb. 1	9 57 16	5 18 31·64	71 10 10·0	10	*s* Tauri	good.

The observations are chiefly made with the Fraunhofer equatorial: they are corrected for parallax, but not for refraction.

Assumed apparent places of stars compared,—

	Mag.		R.A. h m s	N.P.D. ° ′
a	9	Oct. 27	5 5 58·80	76 4 (approx^{t}.)
Bessel Zones iv. 1312	7		4 58 7·74	76 7 15″·0

Deduced from 2 comparisons with B.A.C. 1500. The mean places of the other stars are taken from the catalogues cited.

HAMBURG. Merid. Circle & Transit. (MM. C. & G. Rümker.)

1848.	Hamburg M. T. h m s	R.A. Mer. Circle. ° ′ ″	Transit. ″	Declin. ° ′ ″
Jan. 3	9 20 4·4	62 37 15·7	21·7	+15 59 26·0
4	15 53·3	33 27·0	31·5	16 4 35·8
5	11 44·7	30 16·2	17·2	9 48·3
6	7 38·0	27 35·0	...	15 9·6
7	9 3 34·1	25 33·7	34·3	20 29·3
8	8 59 32·8	24 12·9	11·3	25 50·9
12	43 50·1	24 27·9	...	16 48 10·8
16	8 28 44·0	62 33 52·6	57·0	+17 11 29·2

The transit observations are by M. G. Rümker.

Elliptic Elements by Dr. Brunnow, Director of the Observatory at Bilk, near Düsseldorf.

Epoch 1848, Jan. 0, Berlin Mean Time.

M.		35 24 13·86
π		33 3 5·76
Ω		110 15 2·33
i		5 53 34·24
φ		8 58 43·86
μ		1085·5122
Log a	0·3429133	Sid. Revol. 1193·901 days.

An Ephemeris by Dr. Brunnow, for January, came too late for use.

IRIS.

DURHAM. (Prof. Chevallier & Mr. R. A. Thompson.)

1847.	G.M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	No. of Obs.	Star.
Nov. 16	6 48 57	20 44 46·18	102 29 59·6	3	B.A.C. 7296
17	7 43 50	46 27·54	24 50·7	2	— —
Dec. 8	6 8 22	21 23 19·31		3	B.A.C. 7435
8	6 26 31		100 10 25·4	2	— —

The observations are corrected for refraction, but not for parallax.

Extract of a Letter from Mr. Cooper.

"Mr. Graham, my first assistant at Markree Observatory, compared *Iris*, on the 24th of January last, with a star of 9·10 mag., not to be found in any catalogue we possess. The apparent place of the star on Jan. 23 was found to be

R.A. 22 h 57 m 16·08 s N.P.D. 92 ° 27 ′ 11·1 ″

by 5 micrometrical comparisons with a star in *Bessel's Zones* and in the *Hist. Cél.* As it seems to be variable, the star is worth attention, and its place may be of use to some planet-hunter."

URANUS.

HAMBURG. Meridian Circle. (M. Rümker.)

1848.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
Jan. 4	6 0 3·3	13 27 54·4	+5 4 22·0
5	5 56 10·4	28 27·4	4 35·3
6	5 52 16·2	29 6·3	4 54·6
7	5 48 23·0	13 29 47·1	+5 5 15·0

NEPTUNE.

DURHAM. (Prof. Chevallier & Mr. R. A. Thompson.)

1847.	G.M.T. h m s	R.A. h m s	Obs.-Cal. s	N.P.D. ° ′ ″	Obs.-Cal. ″	No. of Obs.	Star.
Nov. 17	9 23 6	22 0 3·45	−0·90	102 52 2·1	+1·1	5	B.A.C. 7747
19	9 18 34	0 6·11	0·69	51 46·7	0·1	2	— —
1848.							
Jan. 11	5 46 11	22 4 7·11	−0·58	102 29 39·9	+0·1	3	B.A.C. 7722

The observations are corrected for refraction and parallax, assuming the Hor. Equat. Par. = 0″·3. The calculated place is taken from Mr. Adams' ephemeris.

HAMBURG. Equatoreal. (M. Rümker.)

1848.	Hamburg M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
Jan. 9	6 15 35·4	330 58 9·8	−12 30 57·1
16	6 9 5·3	331 10 50·4	−12 26 22·4

Satellite of Neptune.

From a discussion of Mr. Lassell's Observations, and those made at Cambridge, U.S., Dr. Peirce finds the following elements of the Satellite.

Time of Revolution ..... d h m 5 21 12·4

Long. Ascending Node ..... 119°·8 supposing the motion direct.

Inclination ..... 29 ·9

Time of Northern Elongation, 1847, Nov. 26·53, Greenwich M.T.

Greatest Elongation..... 16″·5

Dr. Peirce states the corresponding mass of Neptune to be $\frac{1}{18780}$ of the sun.

MAUVAIS' COMET.

CAMBRIDGE. Northumberland Equatoreal. (Prof. Challis.)

1848.	Greenwich M. T. h m s	R.A. h m s	N.P.D. ° ′ ″
March 3	11 58 6·0	10 50 35·02	71 32 15·9
7	12 46 40·7	10 41 3·39	71 55 45·7

Not corrected for parallax.

March 3 Compared with Hist. Cél. 21083

7 —— 20827 = B.Z. 456, $10^h\ 39^m\ 28^s$

The place of the first star is taken from the Hist. Cél.; that of the second is a mean of the authorities cited (which agree very well), regard being had to the secular variations.

CAMBRIDGE, U.S. Large Equatoreal. (Professor Bond.)

1848.	Cambridge M.T. h m s	R.A. h m s	Dec. ° ′ ″	No. Comp^{s}.	Star comp.
Feb. 29	12 43 55	10 57 23·1	+18 42 48	11	*a*
March 1	15 22 0	10 54 37·8	+18 36 57	12	*a*
	16 37 0	30·2	46	2	*b*

	Mag.	App. R.A. h m s	App. Dec. ° ′ ″	
a	9	10 56 16·84	+18 43 11·5	in Bessel's Zones 456
b	7	10 58 40·53	+18 33 34·2	——

The observed differences of Right Ascension have been applied to the position of the star of comparison referred to the Mean Equinox of Jan. 1, 1848.

On Feb. 25th, at $12^{h}\ 23^{m}\ 30^{s}$, the Comet preceded a star 9 mag. by $4^{m}\ 21^{s}\cdot8$, and was south of it 1′ 8″ by 3 comparisons, using the declination circle of the equatoreal. Approximate place of star,

R.A. $11^{h}\ 11^{m}\ 54^{s}$ Dec. +19° 3′.

Mauvais' Comet was found without difficulty, from its place given by Dr. Peirce's Elements, when in the vicinity of the nebula *h* 843, which it resembled in size and brightness. It was bright enough to allow the use of red illumination and the wire micrometer. There were indications of the stellar point so frequently noticed at its first appearance, and from this, a faint, diffused, nebulous light extends for some distance."

Professor Bond thinks the above observations may be relied upon.

Sweeping Ephemeris for the expected Comet of 1264 *and* 1556.

From Mr. Hind's Tables in the *Notice* for April 1847, p. 264.

Per. Pass.	March 31. R.A. h m	March 31. N.P.D. °	April 10. R.A. h m	April 10. N.P.D. °	April 20. R.A. h m	April 20. N.P.D. °	April 30. R.A. h m	April 30. N.P.D °
1848.								
May 20	9 16	74·1						
30	9 13	82·7	8 29	70·9				
June 9	9 9	86·1	8 43	78·5	8 17	70·2		
19	9 6	88·0	8 47	82·3	8 32	76·4	8 19	70·4
29	9 2	89·2	8 48	84·6	8 37	80·0	8 31	75·4
July 9			8 47	86·1	8 40	82·3	8 36	78·7
19					8 40	84·2	8 38	80·9
29							8 39	82·6

The first vertical column contains the hypothetical date of perihelion passage: the places of the comet corresponding to each hypothesis will be found in the same horizontal line with the perihelion passage, and below the date to which they belong. The R.A.'s are given to minutes, and the N.P.D.'s to tenths of degrees; but the object of the ephemeris is merely to point out the quarter of the heavens in which the comet is to be looked for, and the sweeps should extend pretty widely beyond the data of the ephemeris. If any one should be fortunate enough to catch the comet, it is requested that immediate notice may be given to the Foreign Secretary, Mr. HIND, No. 3 Allsop Terrace, Regent's Park. A similar ephemeris for the whole year, computed by Mr. George Bond, is inserted in the *American Almanac* for 1848, and was communicated to the Society several months ago.

MISS MITCHELL'S COMET.

Elements.

Miss Mitchell has computed the following Elements of the Comet discovered by her on Oct. 1, 1847.

Perihelion Passage, 1847, Nov. 14·499, Greenwich M.T.

	°	′	″
Longitude Perihelion	274	10	6
Node.....	189	35	29
Inclination	71	33	32

Per. Dist. = 0·341348. Motion Retrograde.

Account of the Annular Eclipse of 9th November, 1847.

By Major Lysaght.

Hingolee, at which station the following observations were made, is situated in Lat. 19° 43′ 12″; Long. 77° 7′ 5″ E. The former may be depended upon as correct, but the latter is open to correction.

The weather was very unpromising, and heavy clouds shewed themselves in the forenoon; but a short time previous to the commencement of the eclipse they cleared off towards the western horizon, and left the required part of the heavens beautifully clear and serene, with a light wind.

The observations were made with a Dollond's $3\frac{1}{2}$-foot refractor: 2·7 inches' aperture and power 25, with a coloured glass, which gave the sun a greenish yellow colour. A thermometer was placed against a brick wall with the sun shining directly on its bulb, and a barometer and a spirit thermometer in the shade of an open verandah: the times were noted by an assistant with a chronometer, adjusted to mean time by an observation of the sun in the morning.

The day was exceedingly hot for the season of the year; the exposed thermometer standing at 129°·5 a short time before the commencement of the eclipse, but it fell *immediately after the time of first contact was given* to 92°·2.

Before the commencement of the eclipse the spots on the sun appeared fainter than usual, or than I had observed them a day or two before with a smaller telescope; but the faculæ were very bright. As the moon advanced over the sun, the spots became much darker and well defined, and round the largest (near the centre) was a dark, fine ring, conforming to the shape of the spot, the intervening space being of the same dull colour as the rest of the sun.

The eye having been withdrawn for a little time from the telescope, the *exact* time of first contact was lost by a few seconds.

On nearing the solar spots, the moon, which, up to this period,

was well defined, became undulating, and this undulation increased until the annulus was about to form, when the lower limb again became steady and well defined, and the upper, first much serrated, and then *hillocky*. At first this was attributed to the lunar mountains, but except in two or three places they all subsided. Immediately before the formation of the annulus, and on the western edge, a dark line was seen to connect the limbs of the sun and moon, and gave the idea of a post raised *gradually* from behind *on one* of the *hillocks*, and extending to the sun's limb. This was followed by another; both remained a few seconds, but the manner of their disappearance was not particularly observed; then appeared a thin thread of light, which was noted as the formation of the annulus.

As the ring increased in breadth, the southern limb of the moon became pinnacled, and an instantaneous blaze of light was seen, which had more the appearance of a fire suddenly breaking out on a windy night in dry grass on the summit of a jungly hill (as often seen in this country), than of exploded gunpowder, or any thing else I can liken it to.

The moon's limb again became smooth, leaving a hillock on the eastern side, which, as she advanced, *gradually* elongated, and became pointed, when another post was erected like the former two, but more slowly (so much so that I was nearly turning my eye in another direction, not expecting to see the phenomenon), and remained somewhat longer; this broke in the centre, and the two parts shrunk slowly into the limbs of the sun and moon.

The clouds, which had at the commencement settled down to the western horizon, had again risen, and now approached, and I gave up the hopes of seeing the breaking of the annulus, or end of the eclipse.

There was not much apparent diminution of light during the existence of the annulus, nor were any stars visible, but every thing had a light blue colour.

At $3^h\ 7^m$ P.M., gunpowder was ignited in about two minutes with a lens of 3 inches diameter; but at $3^h\ 23^m$ P.M. it could not be exploded, although the lens was held over it for seven minutes, and care was previously taken to dry the powder.

Before the commencement of the Eclipse.

Barometer	$28^{in}{\cdot}31$	
Thermometer attached	93°	
Ditto exterior	129°5	exposed to the sun.
Time of first contact	$1^h\ 38^m\ 25^s \pm$	
Formation of annulus	3 21 14	

At the middle of the eclipse.

Barometer	$28^{in}{\cdot}27$	
Thermometer attached	89°·7	
" exterior	92	exposed to the sun
Spirit thermometer	84	minimum temperature of air during the eclipse.

The barometer and thermometers are corrected by the standards of the Surveyor-General of India.

Remarkable Appearances during the total Eclipse of the Moon on March 19, 1848.

Extract of a Letter from Mr. Forster, Bruges.

" I wish to call your attention to the fact which I have clearly ascertained, that during the whole of the late lunar eclipse of March 19, the shaded surface presented a luminosity quite unusual, probably about three times the intensity of the mean illumination of an eclipsed lunar disc. The light was of a deep red colour. During the totality of the eclipse, the light and dark places on the face of the moon could be almost as well made out as in an ordinary, dull moonlight night, and the deep red colour, when the sky was clearest, was very remarkable from the contrasted whiteness of the stars. My observations were made with different telescopes, but all presented the same appearance, and the remarkable luminosity struck every one. The British consul of Ghent, *who did not know that there was an eclipse*, wrote to me for an explanation of the blood-red colour of the moon at 9 o'clock.

" The sky was of unusual brilliancy, as often occurs between showers; there was a bright aurora in the north, and a most magnificent meteor descended obliquely towards the north-west horizon about the time of the central eclipse. The western margin of the disc presented a rough, uneven appearance at this time. What would be the effect on the dark surface of the moon of extensive aurora borealis on our earth ?"

Mr. Walkey, who observed the eclipse at Clyst-St. Lawrence, near Collumpton, says the appearances were as usual till 20 minutes to 9. " At that period, and for the space of the next hour, instead of an eclipse, or the umbra of the earth being the cause of the *total* obscurity of the moon, the whole phase of that body became very quickly and *most beautifully illuminated*, and assumed the appearance of the glowing heat of fire from the furnace, rather tinged with a deep red. The above description I gave to the editor of one of the Exeter papers, and some one has attempted to solve the peculiarity of the appearance by speaking of the umbra and penumbra, which might have been the cause of it. But such a solution has nothing to do with the appearance, the whole disk of the moon being as perfect *with light* as if there had been *no eclipse whatever.*

" Having spoken of this appearance, I was informed by one or two individuals that they had seen, between twenty minutes to nine and twenty minutes to ten, a very luminous appearance of the aurora borealis. Now, it strikes me that the light reflected from this northern effulgence might have caused the *luminous appearance* of the moon in this part of the country at the time when it was under the perfect umbra of the earth in other portions of England.

" Many more than threescore years have passed with myself,

and during that period I have several times beheld an eclipse of the moon, but never before did my eyes behold the moon positively giving *good* light from its disk during a total eclipse. The phases of the moon perfectly corresponded with the authorised diagram up to the period of $8^h\ 40^m$ and after $9^h\ 40^m$ to the end of the eclipse."

Occultations of Fixed Stars observed by Mr. Rümker, Hamburg.

	B.A.C.		Hamburg M.T.	
			h m s	
1847. Feb. 25	2486	68 Gemin. *k*	10 27 55·3	Immersion
March 22	1692	115 Tauri	7 22 12·0	—
May 28	5112	35 Libræ ζ^4	10 2 38·8	—
June 22	4521	76 Virginis	8 57 54·6	—
Oct. 26	1526	Tauri	9 25 51·0	Emersion
1848. Jan. 26	1420	87 Tauri *α*	5 16 20·8	—
	By M. G. Rümker		21·5	—

(The nomenclature of the stars has been altered to make it agree with that of the British Association Catalogue.)

Occultations observed at Poona, by W. S. Jacob, Esq.

Lat. 18° 31′ 36″, N.; Approx. Long. $4^h\ 55^m\ 42^s$ E.

1847.
Dec. 13. Occultation of *θ Aquarii*, Poona Sidereal Time.

Immer. $1^h\ 38^m\ 11^s{\cdot}5$ instantaneous; Emer. $2^h\ 45^m\ 37^s{\cdot}0$ instantaneous.

Dec. 16. Occultation of *Saturn*, Poona Sideral Time.

Immer. $3^h\ 40^m\ 7^s{\cdot}2$ *i.e.* total disappearance; Emer. $4^h\ 51^m\ 35^s{\cdot}5$

The planet was not visible at emersion until it was about 2″ separated from the moon's limb, or about 4^s after true emersion: 4^s must, therefore, be substracted from the emersion stated. Planet very dim.

Extract of a Letter from Mr. Maclear.* *(Cape of Good Hope.)*

"I have observed all the stars which Professor Mädler wished me to observe except one which is now coming into visibility. The reductions are proceeding.

"I have got the 46-inch achromatic on the polar axis, and am proceeding with the adjustments. . . . . The dome may be turned with the little finger. There is a little too much side-play, for our violent winds keep it in constant oscillation; but this can easily be checked, and on the whole I am very well satisfied. The practical knowledge I have acquired will, I trust, be useful when the large equatoreal (which is daily expected) arrives. . . . .

"Observations have been commenced, or rather resumed, of the

* See the last *Annual Report*, p. 87.

following list of stars for the detection of annual parallax. *β Hydri, α Phœnicis, α Eridani, α Columbæ, η Argûs, δ, α^1* and *α^2, γ* and *β Crucis, ε, β, α^1* and *α^2 Centauri, α Circini*, B. A. C. 5233, *β, α Trianguli, α Gruis.*"

Observations of α Centauri and other Double Stars made at Poona. By W. S. Jacob, Esq., late Capt. Bengal Engineers.

These consist of several measures of *α Centauri* extending over nearly three years, during which time the angle of position has increased 8°, and the distance diminished 3″. There are observations of 6 other stars, some of which are deemed to be *probably* revolving. The author has appended elements of the orbit of *α Centauri*, a star which is known to have a very sensible parallax.

On the Annual Oscillations of the Level and Azimuthal Errors of the Greenwich and Cambridge Transit Instruments. By Mr. Henry, of the Royal Observatory, Greenwich.

On comparing the level errors of the Greenwich and Cambridge transits for every month through several years, Mr. Henry finds that there is in each instrument an annual variation of considerable regularity. The western pivot, in spring, is always higher relatively to the eastern pivot than at any other season, and lower in autumn. This maximum and minimum coincides pretty nearly with the months of March and September. As the Y adjustments of the Greenwich instrument have not been touched for eleven years (it is believed those of Cambridge have been almost as little disturbed), a tabular statement of the mean level errors for the months of March and September, during the last few years, will shew this variation very distinctly: each level error is the mean of four weekly observations: the sign + signifies that the west end is higher than the east, and — the contrary.

Transit Level Errors.

	GREENWICH.				CAMBRIDGE.		
Years.	March.	Sept.	Annl. Varn.		March.	Sept.	Annl. Varn.
	″	″	″		″	″	″
1836	+2·88	−0·28	3·16	1833	−0·86	−3·30	2·44
7	0·54	0·93	1·47	5	+2·11	1·96	4·07
8	2·32	0·18	2·50	7	−3·14	3·71	0·57
9	1·08	0·61	1·69	8	−3·23	−6·97	3·74
1840	1·64	0·49	2·13	9	+4·10	+1·78	2·32
1	2·21	0·54	2·75	1840	2·62	+1·68	0·94
2	3·03	−0·16	3·19	1	+0·49	−3·12	3·61
3	2·42	+0·93	1·49	1842	−3·18	−5·35	2·17
4	3·78	−0·16	3·94				
1845	+2·76	−0·39	3·15				
		Mean	2·55			Mean	2·48

Thus it appears that both instruments give nearly the same value for the extent of the variation.

Mr. Henry suggests, as a query, whether this annual fluctuation can be attributed to some annual change in the spirit level. This is not likely: first, because we cannot well see how any change in the zero of this instrument should affect reversed results; and, secondly, because any change of scale, if arising from temperature, could scarcely be so consistent in its indications, and, moreover, could not well affect *mean* temperatures so sensibly while imperceptible at *extreme* temperatures. The cause is not obvious, nor is it quite certain, as yet, that the phenomenon is of universal occurrence.

There is a change of similar nature in the azimuthal errors of both instruments, as will be seen from the tabular statement below. The sign + means, that the telescope points to the east of south, or that the west pier and pivot are too much to the south.

Transit Azimuthal Errors.

	GREENWICH.				CAMBRIDGE.		
Years.	March.	Sept.	Annl. Varn.		March.	Sept.	Annl. Varn.
	″	″	″		″	″	″
1841	+3·22	+2·80	0·42	1836	+11·10	+7·30	3·80
2	4·63	2·53	2·10	1840	0·72	−0·70	1·42
3	5·02	2·55	2·47	1	4·77	+1·33	3·44
4	5·73	3·09	2·64	1842	+3·27	+3·09	0·18
1845	+2·92	+1·53	1·39				
			Mean 1·80				Mean 2·21

It seems pretty certain, from the foregoing tables, that the western Y, in both the Greenwich and Cambridge transits, is about 2″·5 higher at the vernal than at the autumnal equinox, and that it is also about 2″ more to the south at the first than at the second season.

On the Interior Satellites of Uranus. By the Rev. W. R. Dawes.

In the *Monthly Notices* for January last were printed some observations of an interior satellite of *Uranus*, which had been made in the autumn of last year by Mr. Lassell and M. Otto Struve. The results are, in several respects, interesting and remarkable. The fact, that one observer always saw the close satellite on the *northern* side of the planet only, while the other as uniformly observed it only on the southern side, is sufficiently curious to invite further investigation.

It is however obvious, that the observations at Starfield and at Poulkova are utterly incompatible with each other. While the latter point to an approximate period of $3^d\ 22^h\ 10^m$, the period indicated by the former is only about $2^d\ 2^h\ 43^m{\cdot}6$. The *distance*

also of the satellite carefully estimated by Mr. Lassell on Nov. 6, 1847, under favourable circumstances and with great probability of considerable accuracy, was only 11″; the position-angle being estimated 349°. Now, assuming the direction of the major axis of the projected orbit to be from 10° to 190°, as determined by M. O. Struve for the satellite observed by him; and assuming also, that the apparent ellipticity of the orbit does not greatly differ from that of the orbits of the bright satellites I. and II.; we find that the distance of the satellite, at its greatest elongation, would be 12″·2, on the supposition that the distance was correctly estimated at 11″ when the position-angle was 349°. But this is almost precisely the greatest elongation theoretically due to a satellite revolving about *Uranus* in the period indicated by Mr. Lassell's observations. We are thus led to the conclusion, that there are at least *two* satellites interior to the nearest bright one: and to avoid the confusion which might arise from applying numbers of any kind to the smaller satellites, I beg permission to denominate them for the present, *a*, *b*, *c*, &c., in the order of distance from the primary;—*a* being the satellite observed by Mr. Lassell, and *c* the satellite observed by M. O. Struve.

M. O. Struve suggests that the satellite observed by him may lose much of its light when in the northern portion of its orbit; and this may be the reason why Mr. Lassell did not see it on Nov. 6, 1847; on which favourable night *c* must have been near its greatest northern elongation. On all the other nights when *a* was observed by Mr. Lassell, *c* was very close to the planet, with one exception only, on Sept. 14; at which time *c* was near its greatest elongation southwards, and might perhaps have been seen if the night had been sufficiently good. Neither Mr. Lassell, however, nor myself then observing with him, perceived any such object in that place.

It should here be stated, that the estimated position for Sept. 14, as given in the printed table, appears to be erroneous. It is inconsistent with the diagrams independently made at the time by Mr. Lassell and myself, which, taking the *measured* position of II. as a guide, shew that the position of *a* was about 80° north *preceding*;—whereas the angle as printed is 80° north *following*. It seems clear, therefore, that the angle should be 350° instead of 10°. This being rectified, and the position-angles computed for the times of observation, by reckoning back from the peculiarly valuable observation of Nov. 6 as an epoch, and assuming a period of $2^{d}\ 2^{h}\ 43^{m}\cdot6$, the estimated angle *minus* the calculated angle comes out, for Sept. $14 = -2°$; for Sept. $27 = -2°$; for Sept. $29 = +1°$; for Oct. $1 = +2°$.

That the satellite *a* should never have been seen by M. O. Struve may arise from its becoming faint in the southern portion of its orbit, as *c* probably does in the *northern*. It appears, from calculation on the assumptions before mentioned, that on every night when M. O. Struve saw *c*, with the only exception of Dec. 10, *a* was in the southern portion of its orbit; yet sufficiently distant

from his observed position of *c* to render any confusion between them impossible. On Nov. 28, indeed, the position-angles of the two satellites must have been almost precisely the same: but the measured distance of *c* was 16″·85; while the distance of *a* could not have exceeded 11″ in that part of its orbit. It should be noticed, with reference to the non-observation at Poulkova of the satellite *a* on Dec. 10, that though on that night near its greatest northern elongation, it was at a smaller distance from the primary than *c* ever was when it was observed. Unless, therefore, the night were unusually fine, so faint an object might easily be overlooked.

So long ago as the autumn of 1845, Mr. Lassell occasionally saw a faint object, supposed to be a satellite, at about the same distance from the planet as *a* was observed to have been last year; and, with only one exception, it was always seen on the *northern* side of the primary, and usually in the north *preceding* quadrant, in which it was uniformly seen in 1847. On Oct. 5, 1845, at $12^h\ 26^m$ Greenwich mean time, being on a visit at Starfield, I had the gratification of seeing this satellite in the 20-foot reflector. When the light of the planet was hidden from the eye by a bar, the satellite became steadily visible; and a careful diagram being made, both by Mr. Lassell and myself independently, the position-angles deduced from them agreed within 3°, the mean being 324°·1. This is very nearly the same with the estimated position on Sept. 27, 1847; and assuming, that in the interval of 721·8896 days, 342 complete revolutions had been performed, the period comes out $2^d\ 2^h\ 39^m\ 36^s$. It seems probable, therefore, that the period of this satellite does not differ much from that quantity.

Supposing that those nights on which the satellites I. and II. were measured at Starfield, in 1847, were probably favourable, though *a* was not noticed, it becomes interesting to know whether *a* were then at such a distance from the planet as might allow it to be visible in the 20-foot reflector. I have, therefore, computed the places of *a* for each of those nights; and the results shew, that only on Oct. 16 was it in the northern portion of its orbit, and not very close to its primary. On every other occasion, therefore, it was probably either invisible in the southern portion of its orbit, or overpowered by its vicinity to the planet.

By a similar computation, it becomes evident, that on none of those nights was the satellite *c* near its greatest southern elongation; and that, therefore, it was probably invisible on the northern side of the planet, or too near it to be discerned.

It is singular, that in the whole series of observations at Starfield and Poulkova, only one night, Nov. 1, is common to them both; and that was of so indifferent a quality at Starfield as to render it improbable that so difficult an object as the satellite *c* could have been detected: *a* was then in the south following quadrant, and rather near the planet, so that it was not seen at either place of observation.

On the whole, therefore, it seems highly probable, that the interior

satellites observed by Mr. Lassell and M. O. Struve are entirely distinct; that the one becomes invisible in the southern part of its orbit, and the other in the northern; and that, during the last apparition of *Uranus*, it has so happened, that each satellite has been seen only by its own observer. If either of them is the same as was discovered by Sir W. Herschel, it seems most likely to be that observed at Poulkova.

The object observed at Starfield on Nov. 6, 1847, and supposed to be an intermediate satellite between I. and II., was estimated to be only 10″ distant from the planet. But its position was then almost precisely *opposite* to that of I., whose distance was estimated at 20″. It is, therefore, obvious, that if this were a satellite, its orbit must be *interior* to that of I. It could not, however, have been *c*, which must at that time have been near its greatest *northern* elongation, and therefore probably invisible. Moreover, its greatest distance could be only about $15''\frac{1}{4}$; and it would, therefore, be nearly intermediate between *a* and *c*; and for the present it may be distinguished as *b*. Though it may seem premature to attempt any conclusions from a single observation of so difficult an object, yet, on the other hand, the observation of Nov. 6 was peculiarly worthy of reliance. The night was unusually fine. The planet was viewed for two hours, during which time the supposed satellites were carried along with it. The distance also of *b* was estimated just half that of I., which was almost exactly opposite to it. And that the estimated distance of I. (20″) was nearly correct, appears from comparing it with the *measured* distance of the same satellite (20″·57) on Oct. 11, when it was in almost precisely the same part of its orbit. If, therefore, it were really a satellite, it appears probable that there are *three* satellites revolving within the orbit of I., at apparent mean distances of about 12″, 15″, and 18″.

Mr. Lassell having favoured me with a communication of all his observations of a close satellite of *Uranus*, with the position-angles estimated at the time, and copies of the diagrams, it appears from them, that only on one occasion has such an attendant been undoubtedly seen on the *southern* side of the planet. This occurred on Sept. 27, 1845, at $12^h\ 6^m$ Greenwich Mean Time. The estimated angle was 160°, the distance was 3 diameters of the planet from the edge of the disk, which gives 14″ for the central distance. This does not at all agree with any probable period of the satellite *a*. But on calculating the period of *c* from this observation, compared with each set obtained by M. O. Struve in 1847, employing as a guide to the number of entire revolutions the approximate period deduced by him, viz. $3^d\ 22^h\ 10^m$, and giving a weight to the Poulkova observations proportional to the number from which each result is derived, the period comes out from the mean of the whole,— $3^d\ 22^h\ 8^m\ 35^s$. Mr Lassell's estimation of distance corresponds to a greatest elongation of about 17″; which, as well as the period, agrees so nearly with the result of the Poulkova observations, as to render it highly probable that the satellite observed by Mr. Lassell on Sept. 27, 1845, is the same as that observed by M. O. Struve.

Extract of a Letter from M. Otto von Struve to the Astronomer Royal.

"You will see from the *Astronomische Nachrichten*, &c., that we have not been idle at Poulkova since your visit. In addition to the published accounts I have little to say, except that my father's calculations of the Great Russian Meridian Arc give a considerably larger value to the difference between the two axes of the earth than has been hitherto found. I cannot tell you the exact quantity, as the calculations are not completed.

"I have finished my observations of the satellites of *Uranus* for this season. As soon as I have a little leisure, I shall deduce their motions from the observations. I have seen the third satellite, distinctly, only once since my communication to Sir John Herschel, viz. at 6 P.M. on January 25th. Its position was 202°, its distance about 18″. I am now inclined to think that the differences of the light of this satellite in different parts of its orbit are so great, that it cannot be seen by our refractor when it is in the opposite direction to that in which I have hitherto observed it. The atmosphere was very favourable on two occasions, when the satellite was supposed to be to the north of the planet, but I could not see the least trace of it. The period of its revolution is, however, somewhat uncertain, for these *negative* observations are far from conclusive.

"M. Döllen has finished his calculations on Bessel's fundamental catalogue for 1820. The result, as respects *Procyon*, is, that the irregularity supposed by Bessel in its proper motion, vanishes altogether."

In a letter to Captain Smyth, Mr. Vallack suggests the utility of adopting the binocular construction in astronomical telescopes, and especially in Newtonian reflectors. He has himself fitted up two mirrors on this principle, and finds a considerable superiority in the pair over a single mirror. Mr. Vallack, if we understand him correctly, proposes the following arrangement. The specula are fixed in parallel tubes, and by raising the further mirror higher up in its tube, and bringing the small mirror or prism nearer to the mirror, an image is formed considerably in advance of the tube, which may be brought so near the image formed by the nearer mirror, that each may be viewed at the same time by its proper eye. The partition between the tubes and also the outer tube must be pierced, to let the rays of the more distant mirror pass out; by a little adjustment, the distance between the images may be made to suit different eyes. He remarks, as an objection to the binocular construction, that the observation will not be convenient, if objects of very different altitudes are to be viewed, but still thinks that it would be worthy of trial in certain cases.

The meetings of the Society are held at 8 P.M. *on the second Friday of each month.*

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